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Art. I.—TREATMENT OF RHEUMATISM BY INOCULATION WITH MORPHIA.

BY Q. GIBBON, M. D.

Salem, N. J., May 19th, 1841.

To Professor Dunglison.

Dear Sir,—A wish to present to the profession what I deem a useful fact, has induced me, though at the risk of intruding upon the time of one with whom I have not the pleasure of a personal acquaintance, to offer for your consideration the following remarks:—

Having seen in your work upon "New Remedies," published some time since in the "Library," an article upon the beneficial effects derived from inoculation with morphia in the treatment of local diseases, I determined to make a trial of the plan in local rheumatism, an affection which I have frequently found great difficulty in managing. I was not long afterward called to treat an obstinate rheumatic affection of the knee-joint, which had resisted the usual general and local means. One quarter of a grain of the sulphate of morphia was inserted, by means of punctures, in the skin over the affected part, twice a day for two days, with the most marked and satisfactory results. The patient, who for two weeks previous had suffered extreme pain upon the slightest motion of the limb, was, at the expiration of this period, able to walk with slight inconvenience, and upon the third day, threw aside the crutch with which he had before hobbled across his room. Friction with stramonium ointment, two or three times repeated during the fourth day, removed all remaining disease in the affected part.

The next case was a rheumatism of the wrist, in which no previous treatment had been practised. In this case, which was recent, two applications of the morphia, upon two succeeding days, so effectually removed the pain and tenderness, as to allow of the free use of the hand on the third day.

The third trial was made upon an obstinate rheumatism of the knee-joint, remaining after the subsidence of general rheumatism. The patient, a robust boy of sixteen, after having been rendered motionless for several days by a severe attack of inflammatory rheumatism of the whole system, recovered under the free use of tart. antim. opium, and colchicum, with the exception of the knee in question, which remained exquisitely painful, and tender upon pressure in one spot of about an inch in diameter upon its inner side. The insertion of a quarter of a grain of morphia, produced in a few hours a decided impression upon the pain, and by the second day, the symp-

toms were so far mitigated as to permit free motion in the part. This patient recovered rapidly without any farther medication.

A fourth case, which had resisted acupuncturation, assisted by the free use of the stramonium ointment, yielded upon the fourth day from the first application of the morphia, the patient expressing much satisfaction at the effects of the remedy.

Since treating the above cases, I have had an opportunity of testing the good effects of morphia in several other instances, and with results which induce me to entertain a very favourable opinion of its remedial powers. Judging from the limited experience which I have as yet had of its application, I should think it best adapted to the recent and active grades of the disease. I think it proper, however, to state that I have met with one case of rheumatism in which, though to all appearance a favourable one for its successful exhibition, the morphia failed in producing its beneficial effects.

For the American Medical Intelligencer.

Art. II.—LUSUS NATURÆ.

To Dr. Robley Dunglison.

Dear Sir,—I send you for publication, should you deem it worthy of an insertion in your valuable medical journal, a hastily drawn up description of a remarkable Lusus Naturæ, which occurred in this county, in Nov. 1840.

Yours, &c.,

Wm. S. Thruston, M. D.

Mathew's Court House, Va., April 20th, 1841.

Medical and physiological works abound with numerous cases of very novel and singular developments; with instances of monstrosities assuming every form and shape, both in the inferior and superior orders of creation. Cases of excess, and deficiency of parts, are found to be recorded in every physiological work; but I doubt exceedingly whether there is so remarkable and novel a case on record in any of the American or European publications, similar, in all its bearings, to the one which is now presented to the medical profession;—one so remarkable in its conformation, or singular in its developments; or one so well calculated to excite the interest and curiosity, to elicit the speculations and opinions of the medical and physiological inquirer after truth, and to fill with astonishment and wonder the superstitious and uninformed spectator, as this admirable specimen of one of nature's freaks.

A free negro woman, in the county of Mathews, Va., was delivered on the night of the 29th of November, 1840, of three children. One of them, a remarkably fine child, is still alive, doing well, and apparently in perfect health and perfect in its conformation. The other two are as large as ordinary twin children generally are, and present the following singular appearance. There are two heads and necks, perfect in their organisation; the head of one rather larger than the other, and somewhat deformed or injured from improper delivery; four upper and two lower extremities, two chests, one abdomen or body, one umbilical cord, and one male organ of generation. There is to all appearance a perfect conformation of parts from the umbilicus to the apex, except about the chest, which is a little remarkable in its formation. They are united sidewise, from the clavicle to the umbilicus; the right chest of one, and the left of the other, appear deficient, or rather they seem to have grown into each other; consequently the remaining portions project out somewhat unnaturally. The right and left arms of each are thrown over each other's shoulders and back, presenting the appearance as if they were embracing. The other two arms hang in their natural position.

From the umbilicus downwards, they are one; and to all appearance there is a perfect development of parts, except the organ of defecation, which is somewhat imperfect, or rather nearer the scrotum than natural. It is thought that they were alive just before birth; they certainly possessed vitality during some portion of their uterine existence, every part being well proportioned in size, &c., and evidently nine months children. There being two chests, it is reasonable to suppose that there must be two hearts, and two sets of pulmonary organs, and one set of digestive apparatus. It seems to me, that so perfect a development of external parts could not have taken place without a corresponding development of the internal organs; or that two distinct children, as they appear to be, from the umbilicus to the head, could not be supported by one circulating system, and one set of pulmonary organs. There being one abdomen, it is not, I presume, dealing too much in speculation, to suppose there must be only one set of intestines or digestive apparatus; certainly if there be two, they must have, or rather open into, one common duct, the rectum. This, however, is mere speculation, as I have submitted them to no anatomical examination, designing at some future day to present them to some medical museum. I have them at present in a perfect state of preservation in alcohol. Could this very singular lusus naturæ have been delivered alive, and I doubt not its practicability, had the case been in the hands of a skilful accoucheur, and lived to maturity, we should have had presented to us the singular anomaly of two beings in one, and supported upon only two lower extremities. The celebrated Siamese twins, who have traversed every portion of the civilised globe, gaining notoriety wherever they have exhibited themselves, attracting the attention of the curious and enlightened, filling with astonishment and wonder the illiterate, and eliciting the speculations and opinions of the medical and physiological members of the profession, will not, I am sure, bear comparison with this remarkable *lusus naturæ*.

The writer of this, together with many members of the medical profession who have had the pleasure of visiting and seeing the numerous cases of monstrosities, both in the medical and other museums in Philadelphia, as also of seeing the noted Siamese twins, fully concur in the opinion that this is a far greater curiosity, and greatly surpasses any specimen they have ever seen or read of, in its singular development. Already different views and opinions have been advanced by the medical gentlemen who have seen it, as to its internal structure and physiological relations. It is indeed amusing to observe the countenances, and to hear the singular expressions and opinions of the numerous uninformed persons who have seen it, as to its uterine development and fœtal existence, and finally its delivery; and surely this is not at all remarkable, for whether it may be attributed to the influence of the maternal imagination on the fœtus in utero, or to accidental changes experienced by the fœtus at some period of uterine existence, or to a primitive defect in the germ, are questions well calculated to puzzle the most profound physiologist to solve, as well as to open a wide field upon which the imaginary and speculative members of the profession may muse with pleasure and interest. The celebrated Siamese twins have already elicited much speculation, and have drawn forth the different views and opinions of most of the distinguished members of the medical profession, both of this country and Europe, in relation to their internal organisation, &c. Whether there exists so great sympathy between their nervous and circulating system, in fact, among all their important organic developments, as to endanger their lives, were the ligamentous membrane that unites them severed, has been a question long mooted by medical men. The writer of this, as before observed, has had the pleasure of seeing this noted curiosity, but, to speak candidly, saw nothing about them calculated to excite very much his curiosity and astonishment, except the fact of their being united by a ligamentous substance, and the remarkable similarity in their countenances and physical conformation, &c. The most singular fact is, their having been delivered

alive, and survived to manhood in a country where the medical, and especially the obstetrical, art must have been, and still is, from the nature of things, in a very rude and imperfect condition; but dame Nature does, and would in a majority of instances, accomplish what the eager physician, either to afford speedily relief to his suffering patient, or to enhance his own reputation, too frequently takes out of her hands. How much more astonishing and remarkable, then, is this little curiosity, than the noted Siamese twins? How many more views and opinions would have been drawn forth from the medical profession, could this rare case of lusus naturæ have been delivered alive, and grown up to manhood?

I have thus, Mr. Editor, given you, and through your valuable periodical to the world, as correct a description of this curiosity as I am capable of doing, and which I deemed a duty I owed to the medical profession. I have endeavoured to guard against giving it any false colouring, or rendering it more mysterious and wonderful than it really is; as it presents itself to me, thus I give it to you. I have avoided as much as possible running into any theoretical or speculative opinions of my own, as to its internal physiological relations. I leave this to you, and to the other distinguished members of the medical faculty, hoping that you will, should you deem this worthy of insertion in your journal, give us your views at length, and oblige

Your obedient servant and pupil,

Wm. L. Thruston, M. D.

Art. III.—ON THE ORDER OF SUCCESSION IN WHICH THE VITAL ACTIONS ARE ARRESTED IN ASPHYXIA.[1]

BY JOHN REID, M. D., F. R. C. P. E.

Lecturer on the Institutes of Medicine, President of the Anatomical Society, &c.

A knowledge of the order of succession in which the vital actions of the body are brought to a stand in asphyxia, is not only useful in elucidating the exact nature of the function of respiration, and in pointing out rules for our guidance in the direction of certain remedial agents, but it may also be brought to bear in an important manner upon the investigations into the general laws of physiology. The inquiry is one, however, of unusual difficulty, from the intimate manner in which the respiration is associated in the higher animals with the other vital functions, and the rapidity and energy of the actions and reactions of these upon each other. In conducting such experiments, it is not only necessary to watch closely every phenomenon which presents itself, however fleeting it may be, but all the varied concomitant circumstances must also be carefully analysed, and, if possible, insulated, with the view of ascertaining how far they might affect the results. Our progress in such investigations must, therefore, always necessarily be slow, frequently vacillating and uncertain. These difficulties were found so perplexing, that I had several times nearly given up the present inquiry in despair; and it was not without much labour, and repeated failures, that I arrived at what I considered satisfactory results.

The two points in the physiology of asphyxia which have of late years principally attracted attention are, the nature of the impediment to the circulation of the blood through the lungs; and the cause of the arrestment of the sensorial functions. A correct knowledge of the manner in which the vital actions are arrested in asphyxia, is supposed to be included in the true explanations of these two facts. The first of these, viz. the impediment to

[1] Ed. Med. and Surg. Journal, April, 1841, p. 437.

the free passage of the blood through the vessels of the lungs, and its consequent stagnation in the right side of the heart, and the large vessels leading to that organ, have been attributed to three causes—the cessation of the mechanical movements of the chest; the effects of the venous blood upon the contractility of the heart; and the difficulty of transmitting the venous blood through the capillaries of the lungs, when the chemical changes which go on there between the blood and the atmospheric air have ceased. The opinion, that the blood in death from asphyxia chiefly accumulates in the right side of the heart and the large vessels leading to it, in consequence of the stoppage of the mechanical movements of the chest, was advocated by Haller.[1] He maintained, that when the lungs were distended with air, as in inspiration, that the blood flowed readily and abundantly through the pulmonary vessels; but, on the other hand, when these organs had collapsed, as in expiration, the pulmonary bloodvessels were so compressed, and their angles rendered so acute, that they became in a great measure impermeable to the blood sent from the right side of the heart.[2] Goodwyn[3] argued, in opposion to the mathematical calculations and reasonings adduced by Haller, that, when the lungs are diminished in their bulk, and the acuteness of the angles of the bloodvessels changed only to the extent which occurs during expiration, the flow of blood through them would not be materially obstructed. He also drew additional arguments in favour of this opinion, from the continuance of the circulation through the lungs, when an amount of fluid was present in the chest sufficient to compress the lung to the extent which occurs in expiration, whether this fluid had been effused in the human species from disease, or induced by artificial means in the lower animals.[4] Goodwyn maintained that the cessation of the circulation in asphyxia was chiefly dependent upon the venous blood failing to excite the contractions of the left side of the heart. "When respiration," he says, "is obstructed, the florid colour of the blood is gradually diminished, and the contractions of the left auricle and ventricle soon cease. The cessation of contraction arises from a defect of a stimulating quantity in the blood itself."[5] The views of Goodwyn were attacked a few years after their promulgation by Coleman[6] and Kite.[7] Both these authors adduced the results of various experiments, to prove that the left side of the heart can contract vigorously upon venous blood;[8] and they also both maintained that they had proved experimentally, that, when the lungs are kept mechanically distended during the process of asphyxia, that the quantity of blood found in the right side after death is not found to preponderate much, if at all, over that contained in the left side.[9] Bichat also furnished abundant evidence to prove that the left side of the heart can contract vigorously upon venous blood. In numerous experiments, he found that when an animal is asphyxiated, black blood at first traverses the lungs to reach the left side of the heart, and may for a short time be projected from a cut artery, with very considerable force; and he further satisfied himself, that the contractions of the heart could be renewed even after they had become quiescent in different kinds of violent deaths, by injecting venous blood along one of the pulmonary veins towards the left side of the heart.[10] Bichat especially dwelt upon the importance of discriminating between the

[1] Elementa Physiologiæ, Tom. iii. Lib. viii. Sect. iv.
[2] Opus cit. Tome iii. p. 246. 1776.
[3] The Connection of Life with Respiration. London, 1788.
[4] Opus cit. p. 40–47.
[5] Opus cit. p. 85.
[6] A Dissertation on Suspended Respiration, 1791.
[7] Essays and Observations, &c. on the Submersion of Animals, &c. 1795.
[8] Coleman, Opus cit. p. 118; and Kite, Opus cit. p. 26, 42, and 44.
[9] Coleman, p. 107 to 116; Kite, "From these experiments, it is evident that a small quantity of blood can pass through the lungs when they are in a state of perfect expiration."—P. 58.
[10] Sur La Vie et La Mort, article sixième, § ii.

effects of asphyxia upon the functions of animal and those of organic life; of ascertaining the priority of the suspension of those two great sets of functions, and the influence which they exerted upon each other. He maintained that the heart's action does not cease because the dark blood transmitted to the left side of the heart cannot excite it to contract, but because the dark blood, by being circulated through the coronary arteries in the muscular tissue of the heart, arrests its contractility. This effect of the dark blood upon the contractility of the heart was, however, regarded by Bichat as only an isolated phenomenon in asphyxia; for he believed that the vitality of all the tissues of the body was equally affected by the circulation of this dark blood, and that the functions of the brain, or the animal functions, were always arrested before those of organic life.[1] He maintained that the accumulation of the blood in the right side of the heart did not depend upon any mechanical obstruction in the bloodvessels of the lungs, but from various other causes, among which he enumerates the obstacles opposed to the force of the already enfeebled right side of the heart, by the effects of the circulation of dark blood in the bronchial arteries, and the cessation of the excitation of the lungs by the atmospheric air,[2] aided by the circumstance that the systemic ventricle can more easily overcome the resistance presented by the capillaries of the body generally, than the veins and pulmonic ventricle can overcome that arising from the capillaries of the lungs. Bichat appears to have entertained doubts whether the circulation of the venous blood through the capillaries of the systemic circulation arrested the vitality of the tissues simply by default of excitation, or by exerting some deleterious influence upon it; for, while discussing its effects upon the brain, he thus expresses himself—"Je ne puis dire si c'est négativement ou positivement que s'exerce son influence: tout ce que je sais, c'est que les fonctions du cerveau sont suspendues par elle." Although Bichat failed in giving the correct explanation of the manner in which the vital actions are arrested in asphyxia, yet there can be no doubt that to him we are indebted for having pointed out the true path by which this knowledge was to be attained. Another important advance was made in the elucidation of asphyxia by the experiments of Dr. David Williams, of Liverpool, and Dr. J. P. Kay. Dr. Williams,[3] in experimenting on this subject, found "that when the chest is laid open immediately after the trachea has been tied during the acme of inspiration, the pulmonary veins soon become empty, while the pulmonary artery continues full." From these experiments, he inferred that, in asphyxia, the blood is obstructed in its passage through the lungs, while its circulation through the other tissues of the body continues; and that the obstruction in the lungs "arises from a deprivation of pure atmospheric air." Dr. Kay, from his numerous experiments,[4] has also arrived at the conclusion, that "the circulation is arrested after respiration ceases; because, from the exclusion of oxygen, and the consequent non-arterialisation of the blood, the minute pulmonary vessels, which usually convey arterial blood, are then incapable of conveying venous blood, which therefore stagnates in the lungs."[5] Dr. Kay believes that this stagnation of blood in the right side of the heart and pulmonary artery, occurs in consequence of venous blood being incapable of exciting the arterial capillary bloodvessels of the lungs. The experiments of Dr. W. F. Edwards[6] upon frogs, and those of Dr. Kay

[1] Car, d'après ce que nous dirons, l'affaiblissement qu' éprouve alors le cœur n'est qu'un symptôme particulier de cette maladie dans laquelle tous les autres organes sont le siége d'une semblable débilité.

[2] "Le défaut de son excitation par l'air vital."

[3] On the Cause and the Effects of an Obstruction of the Blood in the Lungs. Edinburgh Medical and Surgical Journal, Vol. xix. p. 524.

[4] The Physiology, Pathology, and Treatment of Asphyxia, 1834.

[5] Opus cit. p. 181.

[6] De L'Influence des Agens Physiques sur la Vie, p. 9, 1824.

upon warm-blooded animals, have very distinctly proved that the circulation of venous blood in the muscular tissue not only does not exert any deleterious influence upon its contractility, but that this property continues to manifest itself considerably longer when venous blood is allowed to circulate through the vessels of that tissue, than when the circulation of the blood has been entirely arrested.

Though the experiments of Dr. Williams and Kay have demonstrated that in asphyxia the circulation is first brought to a stand by some impediment to its free passage through the lungs, yet we believe that few will feel satisfied, after a careful analysis of them, that they enable us to determine whether this impediment results from the cessation of the respiratory movements of the chest, or from the arrestment of the usual chemical changes between the blood and the atmospheric air—a question of considerable importance in general physiology. When we remember the great influence exerted by the respiratory muscular movements upon the force with which the blood is transmitted along its vessels,—a fact first well illustrated by Hales,[1] afterwards by Bichat[2] and latterly in a more definite manner by Magendie[3] and by Poiseuille,[4]—a degree of uncertainty must always exist in interpreting phenomena observed in experiments upon asphyxia, in which means have not been taken to obtain the extent and value of this influence.

Such a precaution is the more necessary, since it has been ascertained that dark blood passes at first in the usual quantity through the lungs, and is sent with great force and in a full stream from a cut artery; that it is not until the respiratory movements have been considerably impaired that it begins to stagnate in the lungs; and that after death, considerable quantities of dark blood are frequently obtained from the left side of the heart. Coleman found that the relative quantity of blood in the two sides of the heart, after drowning, varied considerably; "sometimes being as 7 to 4, at other times as 5 to 2, or as 12 to 7. So that, as a medium, the proportions of the right one to the left are about 3¼ to 1¾. After hanging, the medium was found to be as 2⅞ to 1⅛.[5]

Professor Alison, with the view of supplying this defect in the theory of asphyxia, performed several times the following experiments.[6] A rabbit was confined in nitrogen gas until its respiratory movements had become laboured, and insensibility was approaching. The animal was then withdrawn as rapidly as possible from the glass jar in which it had been confined, and the brain was suddenly crushed by a blow with a hammer, and the chest was immediately laid open. The quantity of blood found in the right side of the heart preponderated considerably over that in the left, and, as the respiratory movements had not been interrupted until the animal had been deprived of life, and the circulation in a great measure suspended, these experiments are obviously greatly in favour of the opinion, that the accumulation of the blood in and around the right side of the heart is dependent upon the cessation of the chemical changes between the blood and atmospheric air in the lungs, and not upon the arrestment of the mechanical movements of the chest.

It appeared to me that very conclusive evidence might be obtained on this question by a series of experiments performed in the following manner. A tube with a stop-cock was fixed into an opening in the trachea, and one of Poiseuille's hemadynamometers was introduced into the femoral artery, for the purpose of obtaining definite information upon the force with which the blood was transmitted along the arterial system. The stop-cock of the tube

[1] Statical Essays, Vol. ii, p. 1 to 33, 1740.
[2] Sur la Vie et la Mort, article huitième, § ii.
[3] Journal de Physiologie, Tom. i. Leçons sur les Phénomènes Physiques de la Vie.
[4] Journal de Physiologie, Tom. viii. p. 272.
[5] Opus cit. p. 18.
[6] Edinburgh Medical and Surgical Journal, Vol. xlv, p. 103.

in the trachea was then shut, and when the respiratory process had been suspended sufficiently long to cause a decided fall in the column of mercury supported by the blood sent along the femoral artery, a large bladder, full of pure nitrogen with a brass nozzle provided with a stop-cock, was fixed in the tube in the trachea, which it fitted accurately, and both stop-cocks opened. After the effects of the respiration of the nitrogen gas had been ascertained, a bladder of the same size as the other, similarly provided with a nozzle, and full of atmospheric air, was then substituted for the bladder containing the nitrogen, and the results compared. The difference between the effects of the respiration of the nitrogen gas and the atmospheric air was most marked, and of such a nature as could not be mistaken; for while the mercury continued to fall in the instrument during the respiration of the nitrogen gas, it rose very rapidly immediately after the atmospheric air had entered the lungs, and acted upon the blood. In this experiment, the same mechanical movements of the chest which failed to renew the free circulation of the blood through the lungs when nitrogen gas was inspired, rapidly effected that object when atmospheric air was permitted to enter the lungs, even when tried on the same animal, and subsequent to the failure of the nitrogen, and, consequently, at a more advanced stage of the process of asphyxia. This experiment was repeated several times, and when the requisite care was taken to procure and employ pure nitrogen, invariably with the same results.[1]

Before directing the attention of the reader to a table containing the results of one these experiments, it will be necessary to take notice of a very unexpected phenomenon which presented itself, and for a considerable time completely embarrassed and perplexed me. Before commencing these experiments, I conceived from *à priori* reasoning, that when the blood had become dark in the arteries, and the animal functions had been suspended, that the mercury would begin to fall gradually and steadily in the hemadynamometer, and that there would in a short time be a marked depression in the level of the mercury. The mercury, however, actually stood higher in the instrument, and the large arteries became more distended and tense for about two minutes after the animal had become insensible, when the blood in an exposed and unobstructed artery was equally dark as that in the accompanying vein, and when the attempts at respiration were few and imperfect, than before the stop-cock in the trachea was shut, and when the animal was breathing atmospheric air freely. This was so unlooked for, at first sight was so inexplicable, and so much at variance with my preconceived notions on the subject, that I was strongly inclined to believe that there must be some source of fallacy; but after repeating the experiment more than twenty times, and invariably with the same results, I was at last compelled to admit its accuracy. I then began to surmise that this arose from an impediment to the passage of the venous blood through the capillaries of the systemic circulation, similar to that pointed out in the capillaries of the pulmonic circulation, by which the force of the left ventricle was principally concentrated in the arterial system, and on placing a hemadynamometer in the vein of the opposite limb, and comparing its indications with the instrument fixed in the artery, this supposition, as may be seen from the annexed tabular view of the results of one of these experiments, appeared to be verified. This fact may explain how a quantity of blood is retained in the left side of the heart in asphyxia. It was also ascertained, that though the fall of the mercury in the instrument after the animal was nearly asphyxiated took place very gradually at first, it at last fell very rapidly. Suppose, for example, that the mercury in the tube ranged between 4½ and 5 inches in height before the entrance of fresh air into the lungs was prevented, it rose

[1] In the experiments first performed, the mercury rose in the instrument, but the nitrogen was mixed with a quantity of atmospheric air, as was proved by the blood becoming partially arterialised in an exposed artery.

above this when the animal had ceased to struggle; it afterwards fell very gradually to between 3 and 4 inches; and when it had fallen to between 2 and 3 inches, it frequently sunk very rapidly to the original level. When atmospheric air was allowed to enter the lungs after the mercury had sunk low in the instrument, no sooner had the air acted upon the blood in the lungs, than the mercury instantly sprung up several inches, and when the blood had become more perfectly arterialised, it again stood lower, and the range was more limited. The respirations were necessarily much diminished in frequency, also slow and heaving after the stop-cock was opened in an advanced period of the process of asphyxia, and it was remarked, that during each respiratory movement the contractions of the heart were not only performed with increased strength, but likewise with greatly increased frequency. When the animal was breathing freely through the tube in the trachea, was quiescent, and when the blood was fully arterialised, the range of level of the mercury in the tube seldom exceeded half an inch, sometimes not so much. When the stop-cock was shut, no change took place in the range of the mercury during the first half minute: generally before the end of the first minute the animal had begun to struggle, and then the range greatly increased,—rising during each attempt at expiration, and during the struggling of the animal, falling during each attempt at expiration and during quiescence. In some of the experiments, the range of the mercury during these different conditions amounted to about nine inches, and in one experiment to ten inches,—making a most material disproportion in the extent of the pressure upon the inner surface of the arterial system of vessels.

TABLE I.—Showing the changes in inches of the height and range of the mercurial column in the vertical limb of the hemadynamometer in one of the first class of experiments, when the instrument was fixed in the artery only; the intervals of time at which each change occurred, reckoning in half minutes from the commencement of the operation; with remarks on the state of the animal at these respective changes. The depth and height of the mercury marked at the end of each half minute indicated, as near as possible, the extent of the range in the level of the column during that interval of time.

Intervals of Time.	Height of Mercury in the tube attached to the Artery.		Remarks on the state of the Animal.
Minutes.	Depth.	Height.	
		1.0	When the hemadynamometer was adjusted to artery, the mercury stood at this height in the vertical tube of the instrument.
		5.5	At the instant the stop-cock was turned, it was 5.5.
½	4.0	4.5	Stop-cock on trachea shut. Dog quiet.
1	3.0	7.0	Do. The artery becoming a little dark.
1½	3.0	9.0	Do. The artery black. Animal struggling.
2	2.0	12.0	Do. do. Animal struggling violently.
2½	4.0	9.0	Do. do. Animal quiet.
4½	4.0	8.0	Do. do. do.
5	4.0	8.0	Stop-cock on trachea opened, and a bladder filled with nitrogen gas applied.
6	3.0	6.0	Do. do.
		11	Bladder of nitrogen removed, and one filled with atmospheric air applied.
7½	5.0	11.0	Bladder removed. Natural respiration allowed.
8½	5.0	6	Bladder removed. Animal quiet.

TABLE II.—Showing the same conditions in regard to the second class of experiments, in which hemadynamometers were applied to both the artery and vein at the same time.

Intervals of Time.	Height of Mercury in the tube attached To the Artery.		To the Vein.		Remarks on the state of the Animal.
Minutes.	Depth.	Height.	Depth.	Height.	
		0.5		0.0	When the hemadynamometers were adjusted to the vessels, the mercury stood at these heights in the two instruments respectively.
		6.0	5.0	6.0	
½	4.0	5.0		4.0	Respiration natural. Dog quiet.
2½	3.5	5.0		4.0	Stop-cock on trachea shut.
3½	3.0	6.0		3.5	Do.
4	2.0	11.0		12.	Do. Animal struggling. The mercury thrown over the top of venous tube, which was 12 inches high.
4½	5.5	10.0		12.	Mercury stood at top of venous tube.
5½	5.5	9.0		8.0	Do.
6½	5.0	11.0		3.5	Do.
7½	5.0	8.		2.5	Do.
8½	2.5			2.1	Do.

In some of the other experiments, the difference between the height of mercury in the two instruments, when the blood became venous, was not so marked as in this.

In performing these experiments, I derived much valuable assistance from several gentlemen, but more especially from Mr. James Spence and Mr. K. T. Kemp.

We now proceed to examine the explanations which have been given by physiologists of the cause of the arrestment of the sensorial functions in asphyxia. We have already stated that Bichat maintained that the suspension of the sensorial functions was caused by the circulation of venous blood in the arteries of the brain; while Dr. Kay believes that he has proved that it is principally dependent upon a diminished supply of that fluid being sent along the systemic arteries, in consequence of the impediment to the circulation through the lungs, and not because the blood sent to the brain is venous—an opinion somewhat similar to that maintained by John Hunter.[1] The experiments of Dr. Kay, in which he injected, "gradually and gently,"[2] four drams of venous blood into one of the four arteries conveying arterial blood to the brain, through a very small syringe, "having a beak with a capillary bore,"[3] though undoubtedly sufficient to prove the highly unsatisfactory nature of the evidence adduced by Bichat in support of his position, that the sensorial functions are arrested by the circulation of venous blood in the arteries of the brain, cannot, however, be adduced as satisfactory evidence against the doctrine itself. Such an experiment may prove that the transmission of a certain quantity of venous blood along *one* carotid artery is not sufficient to produce cerebral derangement; but it cannot enable us to determine what would be the effect of the passage of venous blood along *all the four* arteries of the brain. We have very frequently watched an exposed

[1] Hunter's Works by Palmer, Vol. iv, p. 168–170.
[2] Opus cit. p. 194.
[3] Opus cit. p. 193.

carotid artery in an animal during the process of asphyxia, and have observed that the blood flowing along it gradually becomes darker and darker; and we were satisfied that considerably more venous blood than in the experiments now referred to, is circulated through the brain for a short time before the animal is seized with convulsions and insensibility. It is evident, then, that, if the suspension of the sensorial functions is caused by the presence of dark blood in the arteries of the brain, it must be circulated in greater quantities, and for a longer time than occurred in these experiments of Bichat and Dr. Kay. Before we can proceed further in this inquiry, it will be necessary that we examine the variations in the quantity and force with which the blood is sent along the arteries, and returned by the veins during the process of asphyxia. We have already stated that the arterial pressure, as ascertained by the hemadynamometer, is very little altered during the first half minute after the entrance of fresh air into the lungs has been suspended; that about the end of the first, or the beginning of the second minute, when the animal commences to struggle, the pressure is greatly increased; and that, generally, for about two minutes after the animal had become insensible, and had consequently ceased to struggle, the pressure was even greater than before the commencement of the operation. It was also repeatedly ascertained, that the venous pressure, as indicated by the hemadynamometer introduced into the jugular and femoral veins, was equally great for a short time after the animal had become insensible, as before the respiration had been suspended. When an artery is cut across, immediately after insensibility has supervened, the blood springs from it in a full stream, and with a force equal to what would occur if arterial blood was circulating in the vessels. The insensibility in asphyxia cannot, therefore, depend upon any diminution in the force with which the blood is sent along the arteries of the brain, nor upon any diminution in the vascular pressure upon that organ. As, however, the frequency of the pulsations in the arteries becomes remarkably diminished before the circulation has been fairly suspended, we are naturally led to inquire if any change in the quantity of blood sent along the arteries of the brain could account for the suspension of its functions. With this view, we performed several experiments upon dogs. A tube, furnished with a stop-cock, was introduced into the trachea, and firmly secured there; the femoral artery was then laid bare, that the changes in the blood might be observed, and the number of pulsations more carefully reckoned. We shall give the details of four of these experiments. After the femoral artery had been laid bare, the pulse ranged from 105 to 120 in a minute, and the respirations were very short and rapid. At the end of the first half minute after the stop-cock was turned, the pulse was 92. At one minute and a half, the pulse was about 120, the animal had begun to struggle, and the blood in the artery was decidedly dark. At the second minute, the blood in the artery was nearly as dark as in the accompanying vein, but, from the struggles of the animal, it was impossible to reckon the pulse. At the end of two minutes and a half, the animal had ceased to struggle, was evidently insensible, and the pulse was 42. At the beginning of the fourth minute, the pulse was still 42. The stop-cock was now opened, and the animal allowed to breathe. When the blood was becoming red in the artery, the pulse was 78. A short time after this, when the animal was rapidly recovering its consciousness, the pulse was 60, and the respirations about 132. In another experiment, the pulse was 80 at the time when the stop-cock was closed. At the end of the first minute, the pulse was 114, and the blood was decidedly darker, and the animal was struggling. At the end of one minute and a half, the animal was struggling, and the blood was nearly as black as in the accompanying vein. At the end of two minutes and a half, the pulse was 60, irregular in frequency—two beats following each other rapidly; the animal had ceased to struggle, and the blood was as dark as in the vein. At the end of the third minute, the pulse was still 60, and irregular. In a third experiment, the pulse was 100 before the stop-cock

was turned. At the end of one minute, the blood was getting dark, the animal had begun to struggle, and the pulse was 120. During the course of the second minute, it struggled violently, and the pulse could not be reckoned. At the end of two minutes and a half, the animal had ceased to struggle, the respirations were few and heaving, and the pulse was 78. At the end of the third minute, the pulse was 60. In a fourth experiment, the pulse ranged from 88 to 96 before the stop-cock was turned. After half a minute, the pulse was 71, and the blood was somewhat darker. After two minutes and a half, the animal had ceased to struggle, the blood was as dark in the artery as in the vein, and the pulse was 70. At the end of the third minute, the efforts at breathing had nearly ceased, and the pulse was 66. In such experiments as these, it is impossible to ascertain the exact frequency of the pulse at the precise moment when the sensorial functions are suspended, in consequence of the struggles and convulsive movements with which this is preceded. Taking, however, all the circumstances of the experiments into account, and combining with them the facts ascertained in those previously detailed, to prove that the arterial and venous pressure is not diminished at the time that the animal has become insensible, we have little difficulty in arriving at the conclusion, that, though the pulse has become less frequent about the time that the insensibility has supervened, yet that this has not taken place to such an extent as to justify the opinion that the arrestment of the sensorial functions depends upon any diminished transmission of blood through the vessels of the brain. If a diminution in the frequency of the pulse, to the extent we have indicated, could produce insensibility, this would frequently present itself during the course of disease, and under other circumstances where nothing approaching to it is observed. It must also be remembered that the pulse, as ascertained before the experiment has been commenced, must have been more frequent than usual, from the terror of the animal. In these experiments, I regarded the animal as in a state of insensibility when the struggles and convulsive movements had ceased. The function of respiration continued for a short time after the suspension of the sensorial functions, but rapidly became enfeebled. The circulation of the dark blood in the vessels of the encephalon, therefore, arrests the functions of the cerebral hemispheres before those of the *medulla oblongata*.

Dr. Kay has performed several experiments,[1] from which he has drawn conclusions very different from those which we have just stated. He found, that when the abdominal aorta was cut across in a rabbit of the ordinary size, "nearly seven drams and three quarters of blood would escape from the divided aorta when respiration was unobstructed." He then proceeded to cut this vessel across at different periods after the admission of fresh air into the lungs was precluded, and found that, when cut across half a minute after this, the blood collected almost equaled what would have escaped, if the free access of air into the lungs had been permitted. In another animal it was cut across after a minute and a half, and five drams of blood escaped; when postponed to two minutes and a half, four drams were collected; and when delayed to the termination of the third minute, only two drams were collected. In judging of the value to be attached to these experiments of Dr. Kay, two circumstances are to be taken into account—the time an animal requires to bleed to death; and the precise time at which the sensorial functions are arrested. As there can be no doubt that an impediment to the circulation through the lungs does not occur in the course of the process of asphyxia, it is, therefore, a matter of considerable importance to ascertain not only the precise time at which the sensorial functions are arrested, but also the average period of time which the blood would continue to flow from a cut artery when the respiration is unobstructed, before we can venture to determine whether there is any relation between the suspension of the sen-

[1] Opus cit. p. 185–88.

sorial functions and the arrested circulation in the lungs. With the view of satisfying myself on these points, the abdominal aorta in a rabbit, breathing naturally, was cut across a little above its bifurcation. The blood continued to flow freely for about one minute; it flowed feebly for another minute; and very feebly for about forty seconds more. In this experiment, two minutes and forty seconds elapsed before the bleeding from the artery had ceased. Though in some subsequent experiments the hemorrhage had ceased in a somewhat shorter time, yet we believe that in the rabbit it seldom stops before two minutes have elapsed. With regard to the other point we have mentioned, viz. the exact period at which the sensorial functions are arrested, this has been most unaccountably overlooked by Dr. Kay. He seems not to have been aware that a dog generally becomes insensible in from two to two minutes and a half, and a rabbit in one minute and a half, after the complete occlusion of air from the lungs, so that experiments such as those he has related, made to ascertain the quantity of blood which flows from a cut artery at periods posterior to the occurrence of the suspension of the sensorial functions, cannot be adduced in explanation of effects which have previously happened. In performing the experiments as I have already mentioned, I took the cessation of the struggles and the convulsive movements of the animal as a test of insensibility. When a ligature is tied tightly around the trachea of a rabbit, the animal moves about nimbly at first, but before one minute and a half have elapsed, it has fallen down in a state of insensibility, and the attempts at respiration are few and heaving. As the manifestation of the functions of the *medulla oblongata*, upon which respiration depends, are not necessarily linked with that of the functions of the cerebral hemispheres or the sensorial functions, it must be evident that, in attempting to discover the cause of the cessation of the mechanical movements of the chest, the frequency of the respirations ought to be attended to, and not the suspension of the sensorial functions. This circumstance has not been overlooked by us in performing these experiments; and we are satisfied that the function of respiration is much enfeebled at a period of the process of asphyxia, when this cannot be explained by any diminution in the quantity of blood sent to the *medulla oblongata*. No doubt, respiratory movements may be observed after the pulsations have been very considerably diminished in frequency, but these have become few in number, and performed at long intervals before this condition of the circulation has been induced; but it is quite possible that the ultimate cessation of the functions of the *medulla oblongata* may be hastened by the diminished quantity of blood sent along the arteries supplying it. If we proceed, therefore, to analyse the experiments of Dr. Kay, bearing in mind the length of time the blood continues to flow from the divided abdominal aorta of a rabbit, and the precise time at which the sensorial functions are arrested in the process of asphyxia, we must arrive at very different conclusions from those which he has deduced from them.

In further confirmation of the views we are advocating, we may appeal to the experience of every practical physician; for he cannot have failed to observe the gradual torpor that frequently creeps over the sensorial functions in severe cases of bronchitis, when an ill-arterialised blood is circulating in the vessels of the brain, and the pulse is still pretty strong at the wrist.

We feel very strongly convinced that Dr. Kay has fallen into another error in stating, that, three minutes after the entrance of air into the lungs had been prevented, the blood in the arteries had assumed the venous hue "still imperfectly;" for, in numerous experiments, various gentlemen who were present all agreed that the colour of the blood in the arteries was as dark as that contained in the accompanying veins at a period anterior to this. The statement of Bichat, that the blood in the arteries exactly resembles venous blood in a minute and a half or two minutes, is, I am satisfied, much nearer the truth.

From the various facts we have mentioned, we have arrived at the con-

clusion, that the suspension of the functions of the encephalon are chiefly, if not entirely, dependent upon the circulation of venous blood in the arteries. We do not, however, maintain that venous blood exerts any noxious influence upon the functions of the nervous texture; but believe that the effects are solely to be attributed to the want of the proper excitation of the organ; for, when the circulation of arterial blood is renewed, its functions rapidly remanifest themselves, provided that this be done within a given time.

We believe, then, that, in asphyxia, the order of succession in which the vital processes are arrested, is as follows:—The venous blood is at first transmitted freely through the lungs, and reaches the left side of the heart, by which it is driven through all the textures of the body. As the blood becomes more venous, its circulation through the vessels of the brain deranges the sensorial functions, and rapidly suspends them, so that the individual becomes unconscious of all external impressions. The functions of the *medulla oblongata* are enfeebled about the same period that the sensorial functions are arrested, but are not fairly suspended for some time longer. Immediately after the sensorial functions are suspended, and the blood has become still more venous, it is transmitted with difficulty through the capillaries of the lungs, and consequently begins to collect in the right side of the heart. A smaller quantity of blood must now necessarily reach the left side of the heart; and this diminution of the quantity of blood sent along the arteries, conjoined with its venous character, and the ultimate arrestment of the circulation, being circumstances incompatible with the manifestation of vitality in the other tissues of the body, general death is sooner or later induced.

The persistence of the muscular contractility after the arrestment of the circulation varies, as we have had frequent opportunities of witnessing, according to the age and strength of the individual, and also in a very marked manner from constitutional causes, which are unknown; and in this way we are able to explain how the heart's action may be renewed a considerable time in some cases after apparent death, while in others all the attempts to restore animation, though commenced shortly after the suspension of the sensorial functions, have failed. It must be obvious, that the first and principal object in the treatment of asphyxia is to restore the circulation through the lungs. If once we succeed in this, and thus renew the heart's action, the arterial blood is again transmitted to the encephalon and the other tissues of the body; the functions of the *medulla oblongata* remanifest themselves; the sensorial functions are gradually restored; and the animal heat returns. The derangement of the functions of the *medulla oblongata* and the sensorial functions are not necessarily coequal in extent, and never in importance, in asphyxia, and this is well observed in some of those cases of death from disease or narcotic poisons, where the process of asphyxia occurs more slowly and gradually. In these it is not unusual to find the sensorial functions nearly or entirely suspended, at a time when the respiration is pretty effectively carried on; and it is evident, from various facts, that the arrestment of the muscular respiratory movements is not dependent upon the suspension of the sensorial functions, but upon those of the *medulla oblongata*.

We shall now proceed to make some remarks upon the increased force with which the blood is sent along the arteries during muscular contraction. It has been proved, as we have already mentioned, that the blood is sent with greater velocity and increased force along the arteries during the contraction of the muscles of the limbs and trunk, as in exercise, and this takes place in a more marked manner during violent attempts at expiration. On the other hand, during violent attempts at inspiration, the pulse becomes less frequent, feeble, and soft. In some of the experiments we performed, as we have already mentioned, the mercury rose as high as the eleventh, and in one to the twelfth inch of the scale attached to the tube, during violent attempts at expiration, and the struggles of the animal; while it fell as low as the second inch, during violent attempts at inspiration. During these

different conditions, the pressure upon the external surface of the heart, and its position in the chest, must be somewhat altered, a certain amount of pressure being applied to its outer surface during expiration, and removed during inspiration; and it recedes deeper into the chest during inspiration, and again comes forward during expiration;[1] but we may safely set those aside as exerting any appreciable influence in the production of the phenomena in question. Müller believes that the increased contractions of the heart, accompanying muscular movements of the trunk and limbs, may be caused by a sympathetic or reflex action—an excitant effect being produced in the filaments of the nerves distributed in the contracting muscles, which, being conveyed inwards to the spinal cord, is reflected upon the heart. As, however, he adduces no direct evidence in favour of this opinion, we do not feel inclined to abandon the old explanation, that this is merely dependent upon the mechanical acceleration of the blood, by the pressure exerted upon the bloodvessels by the surrounding muscles during their contraction, and the more especially as we have witnessed several facts which at least prove that a great part of the phenomena in question may arise from this cause. We have frequently remarked, that when an animal was breathing very rapidly, even above 100 in a minute, through a tube in the trachea, that the mercury did not rise higher in the instrument than before, and that the range was limited, provided the expirations were always short, and, consequently, not attended with much compression of the bloodvessels in the thorax and abdomen. On the other hand, a marked rise of the mercury took place whenever a forced expiration was made, however slowly this was performed. It was also repeatedly observed, that when one instrument was fixed in the femoral artery, and another in the femoral vein of the opposite limb, the mercury stood considerably higher in the instrument fixed in the vein than in that fixed in the artery, when the animal began to struggle violently. In few of the experiments did the mercury rise much above eleven inches in the instrument in the artery, while it frequently ran over the top of a tube twelve inches high, with considerable force, in the instrument fixed in the vein—showing us in some of these experiments a prodigious increase in the pressure upon the inner surface of the venous system, equal to between three and four pounds on every square inch of surface. This greater elevation of the mercury in the instrument fixed in the vein, can only be explained by the effects of the mechanical pressure of the surrounding muscles becoming increased, as the extent of the vascular tubes over which it is exerted becomes elongated, and may afford some indications of the greatly increased impulse communicated to the blood by the powerful pressure exerted by the contraction of the muscles of the chest and abdomen upon their contained bloodvessels, when aided by the contractions of the muscles of the limbs, and favoured by the presence and particular disposition of the valves of those bloodvessels. It is difficult to determine, then, how much this increased flow of blood along the vessels during violent expirations, and during the contraction of the muscles of the limbs, depends upon more forcible contractions of the heart, or upon the mechanical effects of temporary pressure upon the bloodvessels. The increased rapidity and strength of the contractions of the heart during violent expirations, must be partly attributed to the compression of the bloodvessels of the lungs, and the transmission of an increased quantity of blood to the left side of the heart, while the diminution in the strength and frequency of the pulse during inspiration must, in a great measure at least, depend upon the sudden removal of that pressure, so that a great part of the blood propelled during a few of the contractions of the right side of the heart, which immediately succeed the sudden dilatation of the thorax, goes to fill up the bloodvessels of the lungs to that state of

[1] In mentioning this fact in the article Heart, in the Cyclopedia of Anatomy and Physiology, the word inspiration has been inadvertently printed for expiration, and *vice versa*.

plenitude in which they were before the preceding expiration, and a small quantity only reaches the left side of the heart.

We do not think it necessary to make any remarks upon the question, whether or not the blood stagnates in the lungs, in consequence of the cessation of the chemical changes between the blood and the atmospheric air, or upon any supposed effect which the venous blood may have upon the contractility of the capillary vessels of the lungs, as this has already been most ably and most satisfactorily done by Dr. Alison. He has shown that this phenomenon is to be referred to an interesting general law in physiology, which has hitherto not received the attention which its importance demands, by which the movements of nutritious juices is influenced by the chemical changes, or, as he terms them, the vital attractions connected with the chemical changes, which are constantly going on in the capillary vessels between those juices and the surrounding tissues, by which nutrition and secretion are effected. That such a moving power exists, regulating the quantity of blood which flows through each individual organ, independent of any impulse from the living solids, cannot be doubted.[1] Before arterial blood can be transmitted freely through any tissue or organ, it is not only necessary that the contractions of the heart be performed with a certain amount of force, but that the actions of nutrition and secretion be also in operation; so in the same manner, before the blood can be transmitted through the lungs, it is not only necessary that the right side of the heart retains its contractility, but that the chemical changes between the blood and the atmospheric air should proceed. This doctrine is still further illustrated by the fact which we have ascertained, that, when the blood in the systemic circulation becomes decidedly venous, and unfit for carrying on the process of nutrition, it passes less freely through the capillary arteries into the veins.

BIBLIOGRAPHICAL NOTICES.

Robertson on the Teeth.[2]

We have had, in recent times, several excellent treatises on the teeth, but they have been directed rather to the anatomy and physiology of those organs than to their pathological and therapeutical relations.

The author is of opinion that the destruction of the teeth is effected by the corrosive or chemical action of the solid particles of the food, which have been retained, and undergone a process of putrefaction or fermentation, in the several parts of the teeth best adapted for their reception. This we think very questionable. The process of decay certainly commences, in general, in the interior of the tooth, whence it proceeds towards the circumference; and we think there is great reason for the belief, that the decay is rather dependent upon intrinsic than extrinsic causes. The work, however, contains many valuable practical observations, and is adapted both for the professional and the *lay* reader.

[1] Vide Outlines of Physiology, 3d edition, p. 22–25, 61–64, and 224.

[2] A Practical Treatise on the Human Teeth; showing the Causes of their Destruction, and the Means of their Preservation. By William Robertson. With plates. First American from the second London edition. 8vo. pp. 229. Philadelphia, 1841.

Traill's Medical Jurisprudence.[1]

The valuable work of our friend, Professor Beck, has been so long before the profession, that it is not necessary for us to speak of its sterling merits. Still there has been room for a smaller treatise, that may give the "outlines" of the science, leaving it for those who may wish to study the subject profoundly to consult the larger. Dr. Traill has supplied such a desideratum, and has given, in epitome, the leading general principles of medical jurisprudence, and of medical police.

The American editor "has restricted himself to the correction of errors, and to the adding of such notes as appeared to him to throw additional light on the topics discussed in the text."

Sir James Clark on Climate.[2]

The reception which this work has experienced from the profession—as indicated by its being in its third edition—is a sufficient evidence of its worth. The distinguished author has had ample opportunities for testing the sanative agency of different climates, and is possessed of excellent powers for improving those opportunities. Since the work was given into the hands of the compositor, we have seen it advertised for republication in New York, so that it will be placed fairly before the profession of this country. Sir James has had no experience of the climates on this side of the Atlantic, but he indulges in philosophical reflections which may be applied to all.

Tweedie's Library of Medicine.[3]

This volume concludes the Library of Practical Medicine proper, and is equal to any of its predecessors. The encomiums which we have passed upon them, may with much propriety be transferred to this. It contains, also, a general index to the series.

The enterprising publishers have in press a System of Midwifery, from the able pen of Dr. Rigby, and they propose to publish a forthcoming surgical work, as soon as it appears in London, with notes by an American surgeon.

The whole series will be well worthy of a place in the library of the practitioner.

[1] Outlines of a Course of Lectures on Medical Jurisprudence. By Thomas Stewart Traill, M. D., F. R. S. E., &c. &c., Regius Professor of Medical Jurisprudence and Medical Police in the University of Edinburgh. First American from the second Edinburgh edition. Revised, with numerous notes. 8vo. pp. 234. Philad. 1841.

[2] On the Sanative Influence of Climate. By Sir James Clark, Bart., F. R. S.; Physician in Ordinary to her Majesty and to Prince Albert, &c. &c. Third edition. 12mo. pp. 337. Lond. 1841.

[3] Dissertations on Hemorrhages, Dropsy, Rheumatism, Gout, Scrofula, &c. &c., with a Formulary and General Index. By Geo. Burrows, M. D., &c. &c.; Geo. Budd, M. D., &c. &c.; Thomas Watson, M. D., &c. &c.; Thomas Shapter, M. D., &c. &c.; Richard Rowland, M. D., &c. &c.; William Budd, M. D.; Arthur Farre, M. D., &c. &c.; and W. Bruce Joy, M. D., &c. &c. Edited by Alex. Tweedie, M. D., F. R. S., &c. &c. with Notes by W. W. Gerhard, M. D., &c. &c. 8vo. pp. 514. Philad. 1841.

Rauch's Psychology.[1]

To the first edition of this work, we drew the attention of our readers in the last volume of the Intelligencer. The call for a second edition so soon, sufficiently indicates the value that is placed upon it. Since the appearance of the first edition, the talented author has died, leaving a void not easily filled in the respectable college over which he so ably presided.

New York Medical Gazette.

We have received the first number of a new weekly under this title, edited by Dr. William C. Roberts, of New York; and we hail its appearance with pleasure—coming, as it does, so immediately after the decease of the New York Quarterly. The superior advantages of New York for medical instruction have been pointed out most elaborately of late, and we see no reason why she should not possess them; but if such be the fact, there certainly must be lukewarmth in the cause of medical science, when we see the difficulty in establishing a successful medical journal in that city.

The present undertaking, we trust, will succeed better than its predecessors. The number before us contains contributions from Prof. J. B. Beck, of the College of Physicians and Surgeons, and from Dr. J. Kearny Rogers, surgeon to the New York Hospital.

MISCELLANEOUS NOTICES.

Dr. Paine and Dr. Bowditch.—The controversy between these gentlemen, growing out of the work of the former, has closed, and we trust amicably, notwithstanding the war of words that has existed for some time between them. As in every controversy of the kind, the parties have disturbed their own equanimity without making much impression upon those whom they are desirous of convincing; for it rarely happens that the contending parties produce any change upon each other's opinions; and the public generally interest themselves but little in the merits of the question, whilst they are always ready to censure both parties. So satisfied are we that this is the general feeling, that it would be difficult to induce us to engage in literary warfare. Notoriety is sometimes acquired by such displays, but it is unenviable; and therefore undesirable. It rarely happens, too, that the points in dispute remain the same as in the first instance. Language is misunderstood, and clearness of perception largely interfered with, by involuntary prepossessions or insufficient examination. Of this, we have an example in the Rejoinder of Dr. Bowditch to Dr. Paine's Reply, (supplement to Boston Med. Journal, June 16, 1841,) in which Dr. Bowditch states that

[1] Psychology; or a View of the Human Soul, including Anthropology. Adapted for the use of colleges. By Rev. Frederick A. Rauch, D. P., late President of Marshall College, Penn. Second edition, revised and improved. 8vo. pp. 401. New York, 1841.

we have accused him of being too severely personal. His words are—"Moreover, having been accused of being too severely personal, (Dunglison's Journal,) it gratifies me to find that one journal in this country sustains me, (Baltimore Medical and Surgical Journal,)" &c. Our remarks, (Oct. 15, 1840,) were—"The author (Dr Bowditch) is an estimable individual; but his zeal for a favourite preceptor (Louis) has outrun his discretion. The 'remarks' are polemical throughout, and often—what is always to be deprecated—personal." Dr. Bowditch is not accused by us of being "too severely personal;" nor do we find that the Journal, which he quotes, "sustains" him in being so.

We cite this as an evidence merely of the facility with which new points may arise in pending controversies. The matter had now better rest; already, indeed, the points of discussion are forgotten by most persons; and all that remains, is a vague recollection that an angry discussion had been carried on between two respectable individuals, in relation to something connected with M. Louis.

Water Doctors.—We learn that, in Paris, the homœopathist is hiding his diminished head before the water doctor, who is the author of the new system

"to make the vulgar stare,
Till the swoln bubble bursts, and all is air."

It is strange that the established inanity of all empirical pretensions, from those of Dr. Rock and Dr. Solomon downwards, has not taught the public to profit by experience. They are as ready as ever to follow in the wake of every advertising quack. The second number of the New York Gazette animadverts with propriety on the system for notoriety and pelf, pursued by men who are in the ranks of the profession, and who stoop to the vilest of charlatanry, only to be equaled by the vendors of razor strops, bear's grease, &c. The true vehicles for professional communications are the medical journals, and we trust—with the editor of the London Lancet—"that before long, no greater condemnation can attach itself to the reputation of a medical man, than to have his name vaunted, and his deeds praised, in the ignorant effusions of the political journals."

Stewart on the Diseases of Children.—In speaking of this work, recently published, the editor of the Boston Medical and Surgical Journal remarks—"Should the present race of medical critics be sparing of praise, it is quite certain that posterity will do justice to the patience, genius, and professional attainments of the author." We doubt not that this praise is merited; but for ourselves we wish to say, that the reason why we "are sparing of praise" is, that the work has never been sent to us; and we suspect that such is the case with other "medical critics" besides ourselves. It is a common law with editors, that in order for a book to be noticed at all, it must come before them in the legitimate mode.

Medical College of Ohio.—The Annual Catalogue for 1840–41 contains the names of 130 students: of whom there were from Ohio, 95;

Indiana, 9; Illinois, 3; Kentucky, 7; Louisiana, 1; South Carolina, 1; Mississippi, 1; Massachusetts, 2; Virginia, 1; New York, 3; Missouri, 1; Pennsylvania, 5; Canada, 1.

The number of graduates was 31.

William and Mary College, Virginia.—We find by the Annual Catalogue of this old and respectable institution, that Professor Millington undertakes a class for medical instruction, for which he possesses ample means of illustration. The subjects taught in the first session, are anatomy, physiology, materia medica, and pharmacy. In the second session, anatomy of the nerves and organs of sense, pathology and therapeutics, operations of surgery, materia medica and pharmacy continued and concluded. The text books are the same as those used in the principal medical colleges.

Pennsylvania Medical College.—Dr. R. M. Bird has been appointed to the vacancy occasioned by the death of Dr. Colhoun. Dr. Bird is an estimable and accomplished gentleman.

Medical College of the State of South Carolina.—The Annual Report—which contains an able valedictory address to the class, by Prof. Dickson—gives the names of 51 graduates: of whom 41 were from South Carolina; 4 from North Carolina; 4 from Alabama; and 2 from Georgia.

Dr. Geddings has been transferred to the surgical chair vacated by Dr. Wagner, owing to ill health.

Vermont Academy of Medicine.—The Annual Catalogue and Circular of the Vermont Academy of Medicine for the session of March, 1841, contains the names of fifty-four medical students.

Medical Department of Transylvania University.—Dr. E. Bartlett has been appointed to the chair of the theory and practice of medicine, vacated by the able teacher, Dr. N. R. Smith, of Baltimore.

Berkshire Medical School.—Dr. James M'Clintock, Professor of Anatomy in the Vermont Academy of Medicine, has been appointed to the same chair in the Medical School at Pittsfield, Mass.

Poisoning by Solution of Acetate of Lead—Lead found in the Urine.[1]—A young girl of good constitution, driven by despair to suicide, took about an ounce of acetate of lead in solution. She was almost immediately seized with collapse and syncope, and afterwards with vomiting and convulsions. Sugared water, sulphate of magnesia, and sulphate of soda, were given, but she died in twenty-five hours. She voided a large quantity of urine, which M. Villeneuve sent to M. Orfila. Carbonised, treated by nitric acid, and submitted to the tests of the salts of lead, this urine afforded a sensible quantity of lead.

[1] Journal des Connaissances Médico-Chirurgicales, Janvier, 1841; and Brit. and For. Med. Review, July, 1841, p. 249.

On the Employment of Arsenious Acid in Phthisis Pulmonalis. By M. Trousseau.[1]—M. Trousseau is now making trial of the powers of arsenic in phthisis at the Hospital Necker; a means, however, not new, as Dioscorides employed fumigations of the sulphuret of arsenic in the same disease. During some few months, M. Trousseau has submitted eight patients to the action of this agent. In four affected with diarrhœa, the disease continued its progress; it was in an advanced stage, and death occurred in the usual manner. In four others, notwithstanding vast caverns, and symptoms which announced approaching death, the symptoms amended under the influence of arsenious fumigations, the general health improved, appetite returned, digestion was good, emaciation checked, and the cough and expectoration diminished.

A sheet of white paper is dipped in a solution composed of one part of arseniate of soda and thirty parts of water. The paper is then made into little cigars of the length of a finger, and the patient is directed to smoke one, or even two, daily, in such a manner that the fumes may pass into the lung. This is readily accomplished, by the patient inspiring the moment the fumes enter the mouth. This inspiration of arsenious vapour at first causes slight cough, but after some time both cough and expectoration are much diminished.

This means, though it has not as yet cured phthisis, deserves a trial. In one case of chronic catarrh with emphysema, it rapidly removed symptoms of suffocation. Careful experiments should be made with this remedy, for phthisis is a disease hitherto so incurable, that any thing which affords any probability of effecting permament good is worthy of investigation.

On Tincture of Iodine as a topical Application to Phagedenic Chancres. By. M. Ricord.[2]—After a trial of all the most common topical applications recommended in the treatment of phagedenic chancre, M. Ricord finds them all frequently inefficacious, and from none has he obtained such prompt and happy results as from the tincture of iodine. He has employed this tincture during the last three months, and almost constantly obtained a prompt modification of the ulcerated surfaces, which soon lose their phagedenic character. This has occurred in many cases. The action of the iodine was particularly evident in a patient who had an open bubo, the ulceration being phagedenic and very extensive. Various means were employed during two months without success, when the tincture of iodine alone so modified the ulcerated surfaces, that their extent visibly diminished from day to day. The iodine of potassium had been given in this case without effect, but its use was continued conjointly with the tincture of iodine as a topical application. The cure was perfect in less than a month.

Ectropion cured by Autoplasty. By M. Bérard.[3]—M. Bérard presented to the academy a young butcher, whose inferior eyelid, all except the free border, had been destroyed by a malignant pustule. After having arrested the progress of this terrible disease, M. Bérard sought to repair the loss of substance, and prevent greater depression of the free border of the eyelid already affected by ectropion. A portion of integument was taken from the temporal region, and applied just below the inferior eyelid. The superior border of this slip became united to the inferior border of the eyelid, and its inferior border to the superior edge of the malar integuments. The whole

[1] Bulletin Général de Thérapeutique. Février 15 et 28, 1841; and Brit. and For. Med. Review, July, 1841, p. 249.

[2] Ibid. p. 252.

[3] Bulletin de l'Académie Royale de Médecine. Mars 15, 1841; and Brit. and For. Med. Review, July, 1841, p. 252.

was maintained in apposition by simple agglutinatives. Four months after the operation, the wounds are cicatrised, the new eyelid is perfectly vital, and of a tint precisely similar to that ordinarily seen in the situation which it occupies.

Jaundice from Non-Elimination, together with Remarks on the Pathological Condition and Chemical Nature of the Bile. By W. H. Lowe, M. D.[1] —Dr. Lowe rejects the doctrine as purely hypothetical, that when jaundice appears suddenly, and without premonitory symptoms, it is due to spasm, or to the opposite condition, paralysis of the gall-ducts. Jaundice from non-elimination may take place, either with the liver in a state of disease, or when there is no appreciable disease in that organ. Of the latter species, Dr. Lowe furnishes a single case of his own, and refers to others. According to Le Canu, three views are entertained by chemists of the condition of the blood of icteric patients. Some believe that it always contains bile; others, that it contains none, but only a particular colouring matter; others adopt a middle view, and think that the blood of such patients, without containing all the constituents of bile, holds its colouring principles.

Dr. Lowe, without attempting to offer an exact analysis of the bile, gives the result of some experiments of his own, "which have been conducted on a somewhat extensive scale." He endeavours to show that the resin of Gmelin, the picromel of Thenard, and the choleic and choloid acids of Demarcay, are "one and the same thing." We must refer our readers to the author's paper, for the experiments on which he grounds this view.

Observations on Dropsy of the Pericardium. By John Mackenzie, Esq., Surgeon.[2]—The diagnosis of hydrops pericardii is still involved in obscurity and uncertainty. The difficulty connected with this subject arises from the rare occurrence of the disease unassociated with effusion into the pleural cavity, and to the still rarer opportunities of verifying such cases as do occur, by post-mortem examination. Having for several years been attached to a large military hospital in Russia, where chronic inflammation of the pericardium and consequent effusion were frequently met with, and where every facility of examining the bodies of those who died was afforded, I observed that, in those cases where there was co-existing effusion into the pleural cavity, the patients, from the commencement of the disease, could not bear the horizontal posture; and that, where the effusion was confined to the pericardium, the patients preferred to be with the head remarkably low, till general effusion took place, and the breathing became laborious: of these, some preferred to sleep on their face, inclining to the left side, in such a manner as to make the region of the heart the most depending part, whilst others lay on their back. After death, I found in some of these cases that the pericardium occupied nearly the whole of the chest, and contained from six to ten pounds of fluid. In two cases there was not a vestige of the left lung to be found, except a thin layer of cellular substance.

The reason why the horizontal posture was preferred in these cases, appears to be, that the weight of the pericardium was taken off the diaphragm and rested on the spine or the ribs. It is to be observed, that in hydrothorax, when the patient lies with his head low, the water in the chest flows back and presses on the root of the lungs, preventing a fresh ingress of air, and thereby causing dyspnœa and sense of suffocation; and that in hydrops pericardii this cannot take place, in consequence of the water being inclosed in a firm bag, which is bound down in its place by its attachment to the diaphragm. A very singular case of this disease came under my observation,

[1] Edinburgh Medical and Surgical Journal, April, 1841.
[2] Lancet, April 17, 1841.

where the patient could lie in all postures equally well, and experienced no inconvenience whatever; yet after death, which was caused by a sudden attack of inflammation of the right lung, the pericardium was found to occupy the whole of the left side of the chest, from the clavicle to the diaphragm, and contained ten pounds of thick brown fluid.

New Method of employing Iodine in Phthisis. By A. Leigh, M. D., Jersey.[1]—I direct the patient to apply a sufficient quantity of iodine ointment on the ribs, under both axillæ, and to cover the head with the bed-clothes, to breathe the iodine volatilised by the heat of the axillæ; the ointment produces counter-irritation on the skin where it is applied, and is to be repeated according to circumstances. This method has appeared to me to arrest the progress of the disease.

Case of Neuralgia Facialis cured by Operation. By T. T. Lambert, Surgeon, Hull.[2]—E. L., a female, aged sixty-three, had for several years been affected with a most painful condition of the nerves of one side of the face, coming on in paroxysms of the utmost severity, mostly towards night. At various times she had tried most of the usual remedies, without experiencing any thing but temporary alleviation. Under these circumstances, as the branches of the facial nerve seemed to be principally affected, the pain shooting across the cheek down towards the lower jaw, and occasionally upwards towards the temple, I proposed the division of the facial nerve in the parotid gland, after its exit from the foramen. To this the poor woman gladly consented, and I proceeded to perform the operation by making an incision betwixt the mastoid process and the lobe of the ear. My next incision brought me in contact with a nerve, which I supposed, from its situation, to be the auriculus magnus nerve, one of the ascending branches of the cervical plexus. On raising this nerve with my forceps, a state of tension seemed to be produced upon all the nerves of the face which were affected with the pain, and the patient exclaimed that was the place from whence all her pain proceeded. Under these circumstances, as the operation for the division of this nerve is trivial, compared with the division of the facial, and as it was just at hand, I determined to try the effects of its division; and the result was most satisfactory: the woman passed a delightful night, and enjoyed some good sleep. The next day she had a little pain; and from that time to the present she has been perfectly well, except having, on one occasion, a very trifling degree of pain. Four months have elapsed since the performance of this operation; and during this interval the patient has been exposed to the intense inclemency of the recent winter. I am not one of those who would pay too much attention to a solitary case; yet, as a single fact, this seems to me to be important, inasmuch as the means employed have effectually cured, for four months at least, and I hope permanently, a very severe case of neuralgic pain.

On the Use of Splints in Chorea. By G. Southam, Surgeon, Salford.[3]—I was led to try the experiment of confining the affected limb in splints, having frequently observed, in those instances where the convulsions had continued in spite of all remedial means, that if the patient's attention be directed to the deranged muscles, as, for instance, when in the hand and arm, causing him to grasp the sides of a chair, the motions comparatively cease; and this, when frequently repeated, proved a useful auxiliary in

[1] London Medical Gazette, May 28, 1841.
[2] Ibid, March 12, 1841.
[3] Ibid, May 7, 1841.

removing the disease. Upwards of two years ago, a case came under my notice where the convulsions had existed in the hand and arm for six months, though a variety of means had been adopted for their removal: believing them to depend on habit, from the control the patient had over the motions, and his apparently excellent health, I had recourse to the splints. For the first few days, the twitchings, from their violence, caused some difficulty in keeping the splints firm, but they soon diminished in force, and in three weeks had entirely disappeared. I have tried the same treatment in four other cases. In three, after attending to the bowels, I applied the splints, and continued them until a cure was established, which occupied in none more than a month. The convulsions were confined to one of the upper extremities, with slight dragging of the leg of the same side; but as the symptoms left the arm, the power over the leg returned, The other case occurred in a youth fifteen years of age. The convulsions were more severe, and rendered it extremely difficult to keep the splints firm with an ordinary bandage: to obviate this inconvenience, I used the apparatus recommended by Velpeau for fractures, with the addition of small splints to keep the limb extended and at rest during the consolidation of the apparatus. This was removed in a week to allow of exercise; and the severity of the convulsions being subdued, a common bandage and splints were again used. Though the convulsions affected both arm and leg, and splints were only applied to the former, the boy had recovered the use of both extremities in six weeks.

NECROLOGY.

The late Dr. Hope.—We regret to see, by the last number of the British and Foreign Medical Review (July, 1841), that the able author of the work on Diseases of the Heart, which is known by reputation to most, if not all, of our readers, died on the 15th of May last, at the age of 40, of pulmonary consumption.

Dr. M'Nevin.—We announce with regret the death of Wm. James M'Nevin, M. D., of New York, late Professor of Chemistry in the College of Physicians and Surgeons, and Professor of Materia Medica in Rutger's Medical School. He died on the 12th of July, at the advanced age of 79.